PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Are interventions for low-income groups effective in changing healthy eating, physical activity and smoking behaviours? A systematic review and meta-analysis
AUTHORS	Bull, Eleanor; Dombrowski, Stephan; McCleary, Nicola; Johnston, Marie

VERSION 1 - REVIEW

REVIEWER	Jean Adams Newcastle University, UK
REVIEW RETURNED	21-Jul-2014

GENERAL COMMENTS	The authors have addressed the majority of my previous comments. However, I still don't feel that my main concern around whether the focus on "low income" was unnecessarily restrictive has been addressed. The inclusion of information on how each study defined 'low income' is helpful and even illustrative in this context. One study (Armitage 2010) seems to define 'low income' as being in a manual or clerical job. Obviously job role is related to income. But so is area of residence and education. None of these terms have been specifically searched for. The authors appear to be interested in whether behaviour change interventions work for people who are living in conditions of socio-economic deprivation. Defining this as 'when authors have used the term low income' seems unusual. It might be helpful to comment on what biases this might have introduced (e.g. is the term more common used in some contexts rather than others?); and how much relevant research could have been overlooked.

REVIEWER	Yana Vinogradova Nottingham University The United Kingdom
REVIEW RETURNED	01-Sep-2014

GENERAL COMMENTS	1. Abstract. This part of a paper, in particular, should invite the reader in. As currently expressed, it seems too technical to be likely to interest a general reader. It would be improved by having quantitative interpretations of the findings with SMD and 95%CI's in brackets. 2. Page 9 line 49. It would be useful to explain how the data were prepared or standardised before calculation of SMD's, i.e. mean portions of fruits and vegetables is expected to increase but, mean

	% of calories from fat is expected to decrease because of the intervention. Also, it would be useful to let a reader know how physical activity outcomes were standardised. Perhaps it would be useful to give an interpretation of SMD here. 3. Page 12 line5. PA should be given in full as no such abbreviation has been introduced. 4. Point 1 would also apply to Discussion. From a technical perspective, the paper of Bull et al is well written and the search and analysis methods are correct. In my opinion, however, the work is not easily accessible to a general reader with a mainly medical background. The paper would be improved if the authors reworded some parts to give more descriptive 'plain English' interpretations, using figures to support these. Currently it reads rather too much like a report requiring mathematical/statistical knowledge.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1 comments:
*The authors have addressed the majority of my previous comments. However, I still don't feel that my main concern around whether the focus on "low income" was unnecessarily restrictive has been addressed. The inclusion of information on how each study defined 'low income' is helpful and even illustrative in this context. One study (Armitage 2010) seems to define 'low income' as being in a manual or clerical job. Obviously job role is related to income. But so is area of residence and education. None of these terms have been specifically searched for.

The authors appear to be interested in whether behaviour change interventions work for people who are living in conditions of socio-economic deprivation. Defining this as 'when authors have used the term low income' seems unusual. It might be helpful to comment on what biases this might have introduced (e.g. is the term more common used in some contexts rather than others?); and how much relevant research could have been overlooked.

We are happy to learn that most of the previous comments have been addressed satisfactorily by our manuscript.

We agree with the reviewer that although we have stated in the limitations that definitions of low income varied across studies, we have not explicitly highlighted that papers having to use the term low income to be included in the review could be considered a limitation, since relevant papers not using this term may have been missed. However, we would also like to emphasise our reasoning for using this criterion, which was that low-income is widely understood across different countries and health care systems, and is commonly used to refer to low socioeconomic status. Additionally, there was a pragmatic reason in that we updated a previous review which used this inclusion criterion.

It is possible that the term low income may be more commonly used in studies using job role or income level to identify participants with low socioeconomic status, and therefore it is possible that our review may be more likely to capture these types of studies. However, we are not aware of any evidence suggesting that the term low income is used more often in certain types of studies than in others, and our review did identify studies which used a wide range of concepts to target low socioeconomic status, such as area of residence, belonging to certain ethnic groups, belonging to a health clinic serving disadvantaged groups, as well as concepts directly linked to low income, such as indicator of income. We therefore feel that although using the term low income may have been somewhat restrictive, it allowed us to implement a clear, objective and replicable criterion for including studies in the review, while also allowing us to capture studies considering low socioeconomic status

in a variety of ways.

In relation to these comments, we have made the following changes to the limitations section of manuscript:

“Definitions of and thresholds for ‘low-income’ varied somewhat between studies, reflecting the fact that there is no one agreed-upon ‘cut-off’ for low-income. We specified that the term ‘low income’ had to be used to refer to participants for studies to be included, since this is a relevant deprivation indicator in our financial and social context, perhaps more so than others such as education level. However, relevant papers not using this term may have been missed, particularly from some settings (e.g. perhaps a church setting) where income may have been less likely to have been measured than others (e.g. the workplace). Nevertheless, our review did identify studies using a wide range of concepts to target low socioeconomic status, such as area of residence, belonging to certain ethnic groups, belonging to a health clinic serving disadvantaged groups, as well as the concepts directly linked to low income, such as indicator of income. Therefore using the term ‘low income’ allowed us to implement a clear, objective and replicable criterion for including studies in the review, while also allowing us to capture studies considering low socioeconomic status in a variety of ways.” p. 17

We have also added this to the ‘article summary’ section as follows:

“We searched for studies where participants were described as ‘low income’ as this is a financially and socially relevant indicator of deprivation, but relevant papers not using this term may have been missed” p4

Reviewer: 2

*1. Abstract. This part of a paper, in particular, should invite the reader in. As currently expressed, it seems too technical to be likely to interest a general reader. It would be improved by having quantitative interpretations of the findings with SMD and 95%CI’s in brackets.

This has now been changed:

“Results: 35 studies containing 45 interventions with 17,000 participants met inclusion criteria. At post-intervention, effects were positive but small for diet [Standardised Mean Difference (SMD) 0.22, 95%CI 0.14 to 0.29], physical activity [SMD 0.21, 95%CI 0.06 to 0.36] and smoking [relative risk (RR) of 1.59, 95%CI 1.34 to 1.89]. p2-3

We have also replaced the term ‘behaviour’, which may be too technical, with the specific behaviours investigated in the review:

“Conclusions: Behaviour change interventions for low-income groups had small positive effects on healthy eating, physical activity and smoking.” P3

*2. Page 9 line 49. It would be useful to explain how the data were prepared or standardised before calculation of SMD’s, i.e. mean portions of fruits and vegetables is expected to increase but, mean % of calories from fat is expected to decrease because of the intervention. Also, it would be useful to let a reader know how physical activity outcomes were standardised. Perhaps it would be useful to give an interpretation of SMD here.

We have amended the document to reflect the reviewer’s points regarding preparation of reverse scored dietary outcomes and interpretation of SMD. Otherwise, data were entered into the analysis as

reported in the studies: since Hedges' g was applied to standardise the data, we did not comment in the text about how the physical activity outcomes were standardised.

"Data from included studies were meta-analysed in RevMan (Version 5.2) using random effect models. For outcomes where a reduction (e.g. mean percentage calories in fat) signifies a change in a healthy direction, data were reverse-scored before being entered for meta-analysis. For continuous diet and physical activity outcomes, standardised mean differences (SMD) were calculated using Hedges' g.28 to express the difference between the means for the intervention and control groups in standard deviation units" p9-10

*3. Page 12 line5. PA should be given in full as no such abbreviation has been introduced.

This abbreviation has now been taken out of the paper:

"Seven studies tested a physical activity intervention" p12

*4. Point 1 would also apply to Discussion.

Thank you, we agree and have re-worded aspects of the Summary of Evidence, Implications of Findings and also Limitations sections of the discussion such that they are now written in a more descriptive 'plain English' style to interest a general reader. Please see the manuscript with tracked changes for all of the specific examples.

*From a technical perspective, the paper of Bull et al is well written and the search and analysis methods are correct. In my opinion, however, the work is not easily accessible to a general reader with a mainly medical background. The paper would be improved if the authors reworded some parts to give more descriptive 'plain English' interpretations, using figures to support these. Currently it reads rather too much like a report requiring mathematical/statistical knowledge.

This is a very valid point reflected in points 1 and 4 of the comments, and is important if we are to convey the main messages of our paper effectively to BMJ Open's wide readership. Although we have provided more procedural details of the work in the relevant sections in response to the valid points raised by the reviewers (including those reviewing the previous version of this paper submitted to BMJ in February), we have made careful rewordings in the abstract, results and discussion sections of the paper in particular to also inform the more general reader. We hope this improves the readability of the paper whilst maintaining the high standard of rigour.

VERSION 2 – REVIEW

REVIEWER	Yana Vinogradova University of Nottingham United Kingdom
REVIEW RETURNED	04-Nov-2014

GENERAL COMMENTS	The authors have made changes to the sentences which were hard to read and I have no further issues.